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Christopher J. Ferguson

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Video Games During the Time of the Plague

Christopher J. Ferguson 

Department of Psychology, Stetson University, DeLand, Florida

Introduction

In their timely essay, Granic and colleagues (this issue) posit digital media as interwoven with real-life experience and an important and nuanced part of adolescent development. When they were writing this piece, there was no way Granic and her colleagues could have anticipated just how timely their discussion would become. In early 2020, much of the world experienced the spread of the covid19 virus which, among other things, resulted in the shutting down of much face-to-face (f2f) interaction due to social isolation and quarantine. With COVID-19 effectively wiping out much of “real life,” digital media have never before been so crucial to how adolescents (and adults) work, learn, socialize, and develop. As I write this, most countries are only beginning to consider loosening restrictions, and it is unclear when “real life” will return and in what form. Unwittingly and undesired, we’re plunging into the deep end of an unprecedented digital age pool.

This means that digital interactions will inevitably be a larger part of identity development for most youth. Unquestionably, this will also raise some concerns. Do digital media cause negative effects, essentially acting like a built-up toxin the way some scholars (and professional guilds) have suggested? Does identity development occur differently when so much of it must occur through digital versus f2f contexts?

The Good News

As we head into this unprecedented time, I’ll start with what I see to be the good news. Mainly, that our fears of digital media are largely overblown and, grueling as this time is, it will probably move most people to an acceptance that digital media is a net boon to society. On the most obvious level, without digital media, kids would not have been able to stay in school, maintain friendships, or perhaps even distract themselves from stress and fear and manage their mood effectively. But I suspect COVID-19 may help nudge the “digital poison” view further into the past, at least for a generation (panics about new media and technology are a historical reality and there’s always something new to panic over).

Debates about whether too much screen time influences youth mental health persist, of course, and in many

examples can be rather acrimonious (e.g., Orben & Przybylski, 2019a; Twenge & Campbell, 2019). Heat rising from academic debates is neither new nor unique to this field nor, frankly, necessarily a bad thing (the saying sometimes goes some conversations have “more heat than light” though sometimes you need both heat and light to get you through the cold dark night of bad science). Probably some of the consternation comes from the tendency of scholars to overstate data in the most frightening terms such as the infamous “Have Smartphones Destroyed a Generation” essay in the Atlantic (Twenge, 2017) which reminds one of the general adages that if a news headline ends in a question the answer is typically “No.” But this too is nothing new, as scholars in video game science happily linked obscure laboratory studies to mass homicide events for years (Markey, Markey & French, 2015). Perhaps no offenders had a more pernicious impact than professional guilds such as the American Psychological Association and American Academy of Pediatrics which churned out unscientific and misleading policy statements on various media effects for years (Elson et al., 2019). This tendency for social science to demonstrate a consistent technophobia and irresponsibly misconstrue weak and inconclusive research fields to the general public as if they had demonstrated powerful, reliable effects, is not only a stain on the scientific ethics of our fields but also set up many people to be suspicious of technology when they needed it most.

Nonetheless, my impression is that evidence linking digital media to harm in youth is weak at best. There is certainly disagreement among studies. Some longitudinal studies suggest that screens may have some negative impact on youth (e.g., Kleppang, Thurston, Hartz, & Hagquist, 2019; Vernon, Modecki, & Barber, 2018) whereas others do not (Coyne, Rogers, Zurcher, Stockdale, & Booth, 2020; Heffer, Good, Daly, MacDonell, & Willoughby, 2019). Overall, effect sizes are weak even where “statistically significant” and probably not of a level that should be considered hypothesis supportive.

The Perils of Weak Effect Sizes

Part of the struggle in making sense of such a divided research field comes from social science’s larger lack of standards in how to effectively interpret weak effect sizes

that may nonetheless be “statistically significant.” Interpretation standards abound, each of them arbitrary from Cohen’s (1992) generous cutoff of $r=0.10$ and above as “small” (the implication being that effect sizes below that were smaller than small, presumably trivial) to Funder and Ozer’s (2019) even more generous threshold of $r=0.05$, to my own more curmudgeonly suggestion of $r=0.20$ (Ferguson, 2009) as a lower bounds for interpreting results as being of practical importance. But the problem of effect size is fundamental... social science has no incentive to interpret effect sizes carefully or cautiously as, to do so, would mean to admit that most of our science is without practical value.

I express the concern that the recommendations of Funder and Ozer, while certainly well-considered, are likely to do more harm than good to our interpretation of effect size. This is because most readers will ignore the cautious implications of the $r=0.05$ threshold and consider the number itself a gift to interpret almost anything, no matter how weak, as somehow meaningful. Much of Funder and Ozer’s argument appears based on the notion that probabilities increase over time (though if so, why limit things to 0.05 and not 0.01 or 0.0001 if, as *Depeche Mode* once said “everything counts in large amounts”) though it’s not clear that this argument applies to social science research which isn’t examining probabilities of discernible events most of the time. Further, a key phrase in Funder and Ozer’s article highlights the weakness of the argument for an interpretation of tiny effects: “Our analysis is based on a presumption that the effect size in question is, in fact, reliably estimated.” This presumption, I’d argue, is almost always false in psychological research.

The problem is readily apparent in much of digital technology research. Accurate measurement of both digital technology use and outcome variables is a topic of debate (Orben & Przybylski, 2019b). Put simply, our self-report instruments seldom rise to the level of scrutiny to which we should be confident in interpreting the $r=0.05$, or 0.10 or even perhaps the 0.20. Studies of mental health at very least appear to fairly consistently apply clinically validated, standardized instruments, but that is not true for studies of aggression for which unstandardized unvalidated measures prone to questionable researcher practices have been the norm (Elson et al., 2014). When measures are unstandardized and researchers free to select outcomes that fit their hypotheses and ignore those that do not, effect sizes and meta-analyses that draw upon them are virtually meaningless.

Even we were to ignore the issue of measurement error, tiny effect sizes can be falsely created by other issues even with standardized, validated measures. These issues that cause spurious effects include common method variance, demand characteristics (and few studies employ controls for hypothesis guessing), single-responder bias, and mischievous responding (Robinson-Cimpian, 2014). The latter issue occurs when youth fill out extreme responses as a joke and has been demonstrated to cause false correlations (e.g., Fan et al., 2006). These can be controlled for with response

validity questions, but, once again, these are rare in this line of research.

Taken together, it is apparent that social science undoubtedly has a hum of false-positive noise that creates small effect sizes that will become “statistically significant” in large samples. The issue is not whether the effect is of practical significance (though there is that) but whether it is real at all. Much of the last decade has focused on replicability issues in social science (e.g., Simmons, Nelson, & Simonsohn, 2011) and this is a positive development. But our next challenge will come in understanding that, even for some replicable research, these results may be the product of common, systematic issues in the research field and are still effectively false positives. With more data we may be better able to estimate effect sizes below which evidence is unable to support a hypothesis. Some have suggested a threshold of $r=0.10$ (Orben & Przybylski, 2019a), although I suspect to get to a 5% or less error rate similar as to alpha rates, we’ll need to get closer to $r=0.20$.

The Curious Case of Video Game Violence

Back in the early 2000s, just after the 1999 Columbine Massacre, the field of video game violence shifted into high gear. Before then, there was a spattering of studies on the topic, largely returning inconsistent results. Almost overnight after Columbine, scholars began comparing video game violence to smoking and lung cancer and making references to mass shooting events (see Markey, Males, French & Markey, 2015 for a full accounting). However, the data remained inconclusive. For instance, one of the most famous (or notorious) early studies claimed to link violent games to aggression (Anderson & Dill, 2000) with the abstract stating “... laboratory exposure to a graphically violent video game increased aggressive thoughts and behavior.” However, a close look at the data from the study reveals significant inconsistencies. Aggressive behavior was measured four different ways, with only one of these achieving statistical significance. Even this outcome would have been non-significant with a proper Bonferroni correction. Games did not affect feelings of hostility nor ratings of perceived crime and safety (neither outcome mentioned in the abstract). Respondents who played an action game were faster in responding to aggressive-themed words (e.g., murder) by pushing a button (what is sometimes misleadingly called “aggressive thoughts” as in the study abstract), though these types of outcomes were explicitly dismissed as unimportant in a review by the US Supreme Court (*Brown v. Entertainment Merchants Association*, 2011) and I agree. This manuscript had other serious issues including a poor match between the action and nonviolent games (Adachi & Willoughby, 2011). Nonetheless, it has been cited hundreds of times. It is an example of how poor-quality research has sometimes been sold to the public (and other psychologists) as supporting a hypothesis when the results from the actual study do no such thing.

Unfortunately, these impressions have been abetted by professional guilds such as the American Psychological

Association (APA) and American Academy of Pediatrics (AAP). Policy statements released by these organizations have been of poor quality, arguably unethical in their misrepresentation of the research field. The APA released policy statements linking games to aggression in 2005 and 2015 (see APA, 2015) in the latter case based on a meta-analysis including only 18 studies out of a field of dozens. A reexamination of that meta-analysis has concluded that it was poorly done, missing dozens of studies, included 5 of 18 studies that did not, in fact, contrast violent games with nonviolent games and ignoring serious methodological issues known to inflate effect sizes (Ferguson, Copenhaver, & Markey, *in press*). In the reanalysis, for studies employing best-practices, such as standardized and validated measures, careful matching of game conditions, or proper theoretical controls in correlational studies, results did not support the presence of effects on aggression. Put simply, the APA developed a policy statement based on a badly performed meta-analysis. What's more, the APA failed to inform the public about inconsistencies between studies, debates in the field, or methodological issues among studies instead portraying the field as reliable and consistent. This position was retained in 2020 despite the APA's own media psychology division writing an open letter protesting the APA's position (Society of Media Psychology, 2020).

Fortunately, though ignored by the APA, there have been nearly a dozen preregistered studies of video game violence (preregistration cuts down on questionable researcher practices in data analysis, one source of false-positive results). None of these have found evidence for game effects on aggression, aside from one which was inconclusive (see Ferguson, 2020 for an overview). That the APA has been incapable of acknowledging their mistake is the kind of behavior that reduces public confidence in science, even if the APA isn't strictly speaking a science organization.

Similar debates have emerged on the topic of "gaming disorder" a diagnosis proposed by the World Health Organization (WHO). Is "gaming disorder" real, or just a product of moral panic or political pressure from "Asian countries" (in the WHO's own words, see Bean, Nielsen, van Rooij, & Ferguson, 2017)? Opinions among scholars here are more diverse than on the issue of violence, for which most surveys find skepticism among scholars about linking games to societal violence. Some scholars support the existence of gaming disorder, others view it as moral panic. What appears to be true is that symptoms of the disorder, particularly as envisioned by the similar "internet gaming disorder" provided in the DSM-5 perform poorly in distinguishing those who do and don't have psychological problems (Przybylski, Weinstein, & Murayama, 2017). Groups like the WHO probably have hewed too close to a substance-abuse model, falsely considering the issues to be similar. In most cases, pathological gaming appears to be symptomatic, not diagnostic, with issues related to underlying mental disorders such as depression and anxiety, or stress due to social circumstances (Jeong, Ferguson, & Lee, 2019).

The Bad News

The bad news is that decades of stigmatization of video games and other digital technology is undoubtedly causing hesitancy among parents at the moment when digital technology may be most useful. The WHO's decision to create a mental health disorder, despite the absence of a consensus among scholars or clear evidence suggesting the diagnosis was useful is particularly unfortunate in hindsight. Fears of "addiction" with attendant pseudoscientific talking points about dopamine and comparing games to cocaine are likely to cause irrational hostility among some older adults, cutting off youth from opportunities to use games positively.

Digital technology always had the power to be a positive element, if a minor one, in youth development. To be sure, video games were not necessary, and not all youth played them. But during the COVID-19 pandemic, other avenues for youth socialization, whether f2f time, sports, etc., were largely shut down. During the spring of 2020, the WHO appeared to be part of a movement endorsing video games as a means to help youth with socialization and maintain social distancing (Foxnews, 2020). This did not represent a repudiation of "gaming disorder" but nonetheless reveals the WHO themselves may be aware of the dangers of stigmatizing games and digital technology during a time when their utility may be particularly pronounced.

As with all things, the use of digital technology must be balanced with other life responsibilities. Even if we understand that the technophobic positions of the APA and WHO are poorly supported by science and digital technology can be a positive element of youth identity development during COVID-19, that does not mean we should endorse entirely unrestricted use. In 2016 the AAP dropped their ill-fated recommendation of no more than 2 h of screen time for youth over 5 (time restraint recommendations remain for younger children, though support for them is inconsistent). This was a positive development given evidence could not support the AAP's recommendations on the 2-h maximum (Przybylski & Weinstein, 2019). Currently, the recommendation is mainly to have a plan that balances digital technology use with maintaining adequate sleep, exercise and responsibilities related to school or work, much of the former of which may also be through digital technology at the time of this writing.

Identity Development for Youth During COVID-19

During the adolescent years, important processes relate to how youth are developing in terms of their personality, moral beliefs, sexuality, and sense of their place in society and life. It has long been recognized that digital technology use can be a normal and healthy part of this identity development (Olson, 2010). Thinking in terms of motivational factors, much of the motivation for digital technology use naturally focuses on fun. However, it is also recognized that digital technology can also help individuals fulfill psychological needs related to socialization, autonomy and control and competence (Przybylski, Rigby, & Ryan, 2010).

The benefit of digital technology to socialization needs and social development is fairly clear. Cut off from f2f opportunities to socialization, it is critical that youth are able to use digital technology for socialization both with friends and with family members from whom they might be isolated. Depending on the length of social distancing, youth during COVID-19 are facing an unprecedented interruption in socialization. The use of digital technology for youth who struggled with f2f socialization was already well demonstrated (Durkin, 2010). However, digital technology use is becoming essential for all youth at present.

During the COVID-19 pandemic youth (and adults) are likely experiencing an unexpected disruption to the sense of control they have over their lives and the direction of their future. Once again, there may be little practical that individuals can do in real life until the government begins to lift restrictions on movement, work, and school. However, digital technology and video games specifically can be particularly effective in giving youth a sense of autonomy and control, assisting them in meeting autonomy needs that are currently difficult to meet in real life. With digital technology and in games specifically, youth can make decisions and see those decisions come to fruition even if in a fictional world. This can help ease some of the stress that can come from a lack of control.

Competence needs can be the least obvious at present. However, when many youths are engaged in virtual school and potentially frustrated with this forced change or even having difficulty connecting with the required technology. Many students may, as such, experience frustrations related to school as well as reduced work opportunities, decreasing opportunities to get competence needs met. Once again, even if in a fictional world, the ability to develop competencies through digital technology can get these needs met when they are difficult to meet in real life.

Conclusions

Digital technology can be an important aspect of positive youth identity development. That has never been more true than during a season in which a global pandemic has disrupted school, graduations, friendships, and even families. Unfortunately, decades of misinformation by professional guilds may have blocked the adoption of digital technology by families at the moment when such technologies may be most beneficial. However, the widespread adoption of digital technology to maintain healthy youth development is an inevitable during this unusual time. My suspicion is that, as an end result, despite some inevitable continued technophobia, one outcome will be greater comfort with the widespread use of digital technology among developing adolescents and their families.

ORCID

Christopher J. Ferguson  <http://orcid.org/0000-0003-0986-7519>

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